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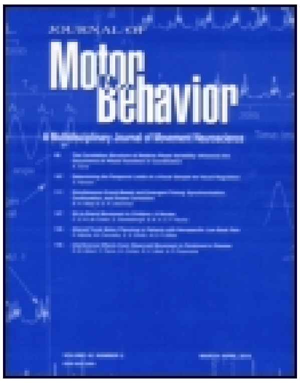

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Rob Bongaardt [a] & Onno G. Meijer [b]

[a] Faculty of Social Sciences and Technological Management, Norwegian University of Science and Technology, Trondheim, Norway

[b] Faculty of Human Movement Sciences, Vrije Universiteit, Amsterdam, The Netherlands

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Bernstein's Theory of Movement Behavior: Historical Development and Contemporary Relevance

Rob Bongaardt
Faculty of Social Sciences and Technological Management
Norwegian University of Science and Technology
Trondheim, Norway

Onno G. Meijer
Faculty of Human Movement Sciences
Vrije Universiteit
Amsterdam, The Netherlands

ABSTRACT. In present-day movement science, N. A. Bernstein's formulation of the problems of motor control is often taken as the starting point. The reliance on Bernstein has not brought agreement among his followers, however. In this article, the authors pose the following question: Does the disagreement arise from the structure of his work itself or from incomplete exploitation of his thinking? By using, *inter alia*, Bernstein's 24 English and German articles, the authors present an analysis of the development of Bernstein's theory of movement behavior, against the backdrop of the scientific progress in the Soviet Union in Bernstein's time and the clashes between Soviet politics and science. Bernstein addressed in his early articles the measurement and biomechanical analysis of movements. His experimental data soon indicated the need for a new understanding of the organization of movements, which he formulated in terms of coordination. Because of political problems, his work was interrupted; but after being "rehabilitated" and again allowed to work, Bernstein aimed to explain how animals find and optimize the solutions to motor problems. The structure of the theory that ensued was comprehensive exactly by virtue of his repeatedly shifting focus between the different aspects of the organization of movement: More important than the answers he gave were the questions he asked. Moreover, the way he approached those questions may help scientists solve pressing problems in present-day movement science.

Key words: N. A. Bernstein, coordination, history of science, motor control, motor planning, movement science, self-organization

In the beginning . . .

> During the process of initiation of a scientist, stories of the heroes of old are told. These "histories" perform several functions. They serve as legitimization myths for the present scientific community, showing it inheriting the mantle of the giants of former times. They help to impress the present dogmas and ideals of the scientific community on the novice, and suggest that past scientific successes have been due to following presently prescribed procedures. . . . Thus, for the most part, the potted histories which preface many textbooks are, not to put too fine a point on it, pieces of methodological propaganda, which rarely stand up to serious historical scrutiny (Powers, 1982, pp. 75–76).

"Reading Bernstein is somewhat like reading the bible," wrote Schmidt (1988, p. 22), paraphrasing Requin, Semjen, and Bonnet (1984). Schmidt ventured that opinion during a conference on the *motor–action controversy* (Meijer & Roth, 1988), where he defended *motor program* versus *action* approaches to movement behavior.

The polemics concerning those approaches started in 1982, when Reed published his theory of action systems. Taking as a starting point Gibson's (e.g., 1979) ecological theory of perception and Bernstein's (e.g., 1967c) theory of movement behavior, Reed emphasized the importance of organism–environment relationships and acidly rejected the notion of motor program, calling it the latest incarnation of dualism (cf. also Turvey, 1977). Thus, Gibson and Bernstein became major sources of inspiration for the ecological approach to perception and action.

During the conference on the motor–action controversy, Schmidt argued that Bernstein's work can also be seen as a source of inspiration to motor program theories of movement behavior. He thereby rejected the exclusive claim of ecological psychologists on Bernstein as their founding father. Implicitly, Schmidt criticized mythological reliance on Bernstein.

Mythologies help students to identify with a particular science. In scientific debates, however, mythologies stand in the way. By highlighting solutions to the problems of bygone days, mythologies obscure the doubts of the founders

Correspondence address: Rob Bongaardt, Department of Psychology, NTNU, N-7491, Trondheim, Norway. E-mail address: bongaardt@svt.ntnu.no

themselves, thereby depriving the scientific community of a rich source of inspiration (Powers, 1982).

In the polemics surrounding the motor-action controversy, mythological elements may indeed have confounded our understanding of the work of Bernstein. In the histories discussed in the prefaces of ecological publications, for example, Bernstein's conception of coordination has often been presented as the starting point for understanding movement behavior (Bernstein, 1935/1967e; cf., e.g., Turvey, 1990). At the same time, Bernstein's theory of the motor program appears to offer an early expression of the ideas that ecological psychologists so strongly reject (Bernstein, 1957/1967f; cf., e.g., Schmidt, 1982).

Whereas ecological and program approaches to movement behavior have been represented as being mutually exclusive, to Bernstein there was no conflict between programs and organism-environment relationships. That idea will become apparent later in the present article, when we discuss the development of Bernstein's work in its context.

In contemporary literature, one finds a growing reliance on Bernstein's work (Feigenberg & Latash, 1996). Nevertheless, our earlier comments illustrate how easily such reliance leads to divergent, or even conflicting interpretations. We contend that understanding Bernstein's intellectual heritage is important in its own right. In the present article, therefore, we attempt to answer the following two questions: What is the main thrust of Bernstein's theory of movement behavior, and what is the relevance of his work to the contemporary sciences of movement?

Bernstein's Translated Publications

Ideally, the development of Bernstein's work should be analyzed by historians of movement science who are familiar with contemporary debates, have a mastery of the Russian language, and have access to all his written work. We do not meet the latter two criteria, and to overcome that limitation, we interviewed several of Bernstein's colleagues.

Of Bernstein's more than 140 publications (Feigenberg, 1988), 24 have been published in, or translated into, English or German. Eight of the 24 appeared in Bernstein's (1967c) *The Co-ordination and Regulation of Movements*, and reappeared in Whiting's (1984) *Human Motor Actions: Bernstein Reassessed.* Bernstein himself was involved in preparing the 1967 book (cf. Bernstein, 1967d) and probably revised some of its chapters (A. G. Fel'dman, March 1995, personal communication). In 1975, Pickenhain and Schnabel published a German version of Bernstein's 1967 book with 3 additional articles, 1 of which was replaced by another article in the 1988, second edition. The latter article can also be found in M. L. Latash's translation of Bernstein's book *On Dexterity and Its Development* (Bernstein, 1991/1996; cf. Latash & Latash, 1994). In 1987, Pickenhain translated Feigenberg's (1978) edition of some of Bernstein's research memos, and 4 articles appeared in 1998 in the journal *Motor Control*, in a special section titled, "The Bernstein Heritage." The remaining 7 articles can be found as original publications in German or English sources.

The Structure of a Lifetime's Work

To avoid the pitfall of misrepresenting Bernstein's thinking by using only today's perspective (so-called Whiggish historiography; cf. Olby, Cantor, Christie, & Hodge, 1990), we present here the development of his theory of movement behavior within its own scientific context. Because we believe that the historiography of movement science should contribute to the movement sciences of today (cf. Provine, 1971), we also discuss the relevance of Bernstein's work to contemporary movement sciences.

Even with only 24 publications at hand, one can easily lose the overview of Bernstein's work unless one orders the material according to some criterion. Most existing orderings are based, each in a somewhat different way, on the scientific content of his articles (cf. Gel'fand, Gurfinkel, Fomin, & Tsetlin, 1971; Gurfinkel, 1988; Luria, 1987; Pickenhain & Schnabel, 1988). By ignoring the context, however, one runs the risk of missing relevant aspects of the content. The order of events in Bernstein's scientific biography therefore offers a more appropriate structure for the present article. Although the accessible biographical information about Bernstein is sketchy, it permits one to distinguish different periods in Bernstein's work (cf. Feigenberg & Latash, 1996; Gurfinkel, 1988; Kozulin, 1984; Latash & Latash, 1994; Luria, 1967, 1987; Pickenhain & Schnabel, 1988).

Nikolai Aleksandrovitsch Bernstein was born in Moscow in 1896. His father was a famous psychiatrist; an uncle was a famous mathematician. In 1914, Bernstein enrolled in the historical-philological program of Moscow University, and he switched to medicine the following year. Having completed his studies after the Russian Revolution, Bernstein enlisted in the Red Army and served therein from 1919 to 1921. Thereafter, he worked as a neuropsychologist, meanwhile attending mathematical and musicological lectures at Moscow University. In 1922, Bernstein was invited to conduct research at the Laboratory for Biomechanics at the Central Labor Institute in Moscow. At the time of his appointment, the institute was in turmoil (Bailes, 1977). Its founder and director, Gastev, had been challenged to pay more attention to the everyday working conditions of laborers. In 1924, Gastev was able to resolve the conflict diplomatically. With hindsight, one can see that political influence on the development of science was comparatively mild in the Soviet Union of the 1920s.

That situation changed around 1930, when the Congress on Human Behavior explicitly rejected all *mechanicism*, specifically Bechterev's reflexology. From then on, neuropsychology had to be dialectically materialist (Kozulin, 1984). Bernstein himself did not run into problems with the authorities, but in the purges that followed, the Central Labor Institute was closed down in 1938, and Gastev was killed (Bailes, 1977). By that time, Bernstein had already moved to another institute. From the late 1930s until 1950, he worked as head of the Laboratory for Biomechanics of the Central Scientific Institute of Physical Culture.

In 1948, at a carefully prepared biological session of the Academy of Science, it was announced that all biological sciences had to be canonized (Kozulin, 1984). Lysenko took responsibility for genetics; and in another wave of purges, Soviet intellectuals were terrorized. In the 1950 Joint Session of the Academy of Science and the Academy of Medical Sciences, *Pavlovianism* (or, rather, Soviet neo-Pavlovianism, which is the doctrine that Pavlov's laws of the conditioned reflex explain human functioning) was declared the official doctrine of neuropsychology. Adversaries were urged to "acknowledge their errors in print" (Brushlinskii, 1993, p. 86; cf. Payne, 1968). At that time, Bernstein was considered a public enemy, and he was fired from his job. After Stalin's death in 1953, Bernstein was gradually "rehabilitated" and allowed to work as a senior scientist. Although he officially retired in 1956, he remained involved in the work of his students and colleagues.

In the present article, we follow Kozulin's (1984) lead and use the clashes between politics and science in the Soviet Union as major landmarks in Bernstein's scientific biography. Accordingly, we have organized our analysis into three sections: The first covers Bernstein's work during the build-up of Soviet science (1922–1930) and was discussed in part in the preceding section; the second covers the period of "political correctness" (1930–1948); and the third covers Bernstein's work after the neo-Pavlovian craze (1954–1966). Each of the sections is introduced by a sketch of the relevant context.

The Build-up of Soviet Science (1922–1930)

In 1918, Lenin challenged scientists to increase labor efficiency in the Soviet Union and to ensure "the elimination of redundant and inefficient movements, and the formulation of the correct methods of labor" (Lenin, 1918/1970, p. 249; cf. Pickenhain & Schnabel, 1988). Lenin decreed that two existing laboratories for the study of labor had to merge into one Central Labor Institute. The merger took place in August, 1921 (Bailes, 1977).

Earlier, around 1900, industrial engineers in the United States had begun to contribute to the success of mass production by providing management advice (cf. Rabinbach, 1990). Taylor (cf, e.g., Taylor, 1913) recommended that managers use a stopwatch to clock the time that a skilled laborer needed to accomplish any elementary operation. Soon, however, the scientific value of selecting the pace of the fastest worker as an absolute standard for all was criticized. Gilbreth and Gilbreth-Moller (1918) proposed that industrial labor efficiency be viewed in a wider context. In their analysis of workers' movements, they also relied on the trajectories of the movements, thereby borrowing heavily from the French physiologist-inventor Marey.

Gastev, the director of the Central Labor Institute in Moscow, found his major inspiration in *Taylorism* (Bailes, 1977). Gastev's dream for Soviet industry was "mechanized collectivism," in which "there is no longer any individual face but only regular, uniform steps and faces devoid of expression . . . , measured not by a shout or a smile but by . . . a speed gauge" (Gastev, 1919; cited in Bailes, 1977, p. 378). Both within and outside the Institute, Gastev was attacked for his uncritical reliance on Taylor, his disregard of physiology and psychology, and his narrow-minded focus on isolated movements. To resolve the conflict among scientists, the Second All-Union Conference on Scientific Management was held in March, 1924. Gastev, urging forbearance, convinced the delegates that his recently adopted policy to attract multidisciplinary expertise would enable him to stem the criticism. The conference attendees acceded to his entreaties and accepted the ideas promoted at the Institute as fundamental to the economy of the Soviet Union.

In 1922, Bernstein was invited to work in the biomechanics laboratory of the Central Labor Institute. At the time, he was working as a neuropsychologist; he accepted the invitation, perhaps because he wanted to further his understanding of the brain through the study of movement.

Throughout his career, Bernstein always collaborated with or stimulated collaboration with other research groups. In 1924, he was listed as one of the co-workers at Kornilov's Moscow Institute of Experimental Psychology. He frequently cited the Gilbreths and even copied some of their work. In this period, however, his focus was on biomechanical aspects of human movement.

In the 1920s, the most pressing problem in the sciences of movement was how to make accurate measurements. Bernstein was directed to that problem by his reading of the six volumes of Braune and Fischer's *Human Walking* (1895–1904). Braune and Fischer had credited *Mechanics of the Human Walking Apparatus* (Weber & Weber, 1836/1894, 1894/1992; cf. Bernstein, 1992) as the classic study of human walking. In their time, the Webers modeled the freely swinging leg as a jointed pendulum and suggested that men could walk more efficiently by exploiting the pendulum properties of their legs (Flesher, 1997). Braune and Fischer (1895–1904) disagreed: Pendulum models are not adequate because the legs are always under the control of voluntary muscle action, they argued. They also claimed that one could experimentally resolve that issue by performing precise measurements.

Several innovative techniques for the measurement of walking had been developed by the Weber brothers in the 1830s. They had measured, for instance, the inclination of the trunk by adjusting a cross-hair in their telescope. In the 1870s, Marey developed *myography* and *odography*, techniques with which displacements of body parts are directly registered on a rotating drum (Marey, 1878; cf. Braun, 1992). In cases where those techniques could not be applied, Marey used sequences of photographic images on a single glass plate. In 1888, replacement of the glass plate by a movable roll of light-sensitive material led to *chronophotography*, a technique that avoided the problem of overlapping images.

In their studies of human walking, Braune and Fischer (1895–1904) adopted Marey's techniques. Sources of light, which could be filmed, were attached to the body of a walking person. To allow for biomechanical analysis, Braune and Fischer (1889/1984) calculated masses and centers of gravity from cadaver specimens. Their mechanistic understanding of human gait was considered state of the art for decades to come. Thus, when Bernstein entered the field of movement, it was clear that progress would be critically dependent upon the further development of measurement techniques.

Measuring Details

The year before Bernstein entered the Central Labor Institute, he attended lectures on mathematics and musicology, which clearly influenced his work. The majority of his articles in the first period under discussion (1920–1930) were devoted to the problem of measuring movement with sufficient precision. In three articles in the second period (1930–1948), he referred directly back to the earlier work (i.e., Bernstein, 1934/1967g, 1936; Bernstein & Dementjeff, 1933). Although those three articles were written later in the period, we discuss them in the present section.

In 1927, Bernstein presented a method for analyzing irregular signals with a decreasing amplitude (*convergent aperiodic trigonometric series*). The irregularity, he suggested, may result from the superimposition of regular rhythms that have no rational relationships. By developing a Taylor series of the signal, one can infer the partial period of each constituent rhythm as well as its corresponding amplitude and relative phase. That method, Bernstein (1927a) contended, could be applied to acoustics, gait analysis, industrial labor, or, for instance, movement pathology.

In cooperation with the Institute for Music Sciences, Bernstein developed a motion picture camera, the *kymocyclograph*, to "extract the maximal available amount of information about the process of movement" (Bernstein, 1934/1967g, p. 7; cf. Bernstein, 1927b). A combination of specific technical features distinguished kymocyclography from comparable methods. First, it allowed for great spatiotemporal precision in the measurement of movements; second, it did not interfere with the ongoing movements of the subjects; and third, by including both a temporal and a spatial frame of reference, it allowed for swift data analysis (Bernstein, 1927b, 1936).

In kymocyclography, different film sizes (3.4–12.0 cm) could be used. The film moved evenly and with adjustable speed (0.6–50 mm). One could adjust the film's movement frequency (from 60 Hz up to 500 Hz; Bernstein & Popova, 1929) by using a rotating disk with holes, so that the filmed movement could be broken into segments; the disk thus functioned as a high-speed shutter. When air was blown through holes in the disk, a siren-like sound was emitted. One could then determine the speed of the disk by using a pitch fork that produced the same tone. From that speed, a time frame could be obtained that was sufficiently precise even for slow-motion analysis (Bernstein & Dementjeff, 1933). One could create a spatial frame of reference by continuously filming a measuring rod in the background. Simultaneous recordings of the mirror reflection of the movement allowed for three-dimensional analysis (Bernstein, 1930). All in all, one could accommodate the measurements to almost any experimental set-up. "In order to add 'flesh' to the picture" (Kozulin, 1984, p. 63), Bernstein used living humans to determine the centers of gravity of body parts (cf. Bernstein, 1936).

Those technical and methodological efforts all served to allow scientists to progress from studying the *what* of movement (trajectories) to the *how* ("analysis of underlying mechanisms"; Bernstein & Popova, 1929, p. 397). Bernstein wished to calculate velocity from position, acceleration from velocity, and force from acceleration in order to infer the central signal from force. He derived that approach from the mechanicism that characterized Braune and Fischer's work. In that vein, Bernstein claimed that the results of kymocyclographic analysis revealed "with great clarity the high degree of automation . . . mechanical simplicity and lawful structure" (Bernstein, 1927b, p. 789 [translated by R. B. and O. M.]) of, for instance, the filing movements of a skilled laborer.

Thus, the innovations in Bernstein's first period were of a technological, not a conceptual, nature. There were, however, signs of a more sophisticated style of reasoning. In a 1929 article on piano playing, Bernstein and Popova showed that relationships within the neuromotor system are not as straightforward as Braune and Fischer would have predicted. To infer the central signal one must know more than just the force, Bernstein and Popova argued, because torque depends on the length of the lever arm. More significant, the same "tone force" can be realized by different constellations of mass and velocity. The movements of piano players are indicative of such "non-univocality". If one varies the pace of the movements, different forms of organization emerge:

> During slow tempos, the movement consists of isolated impulses; during medium tempos, the movement corresponds to the oscillations of a compound pendulum; during the fastest tempos, there is a transition to forced elastic oscillations of a simple pendulum (Bernstein & Popova, 1929, p. 432 [authors' translation]).

The Period of "Political Correctness" (1930–1948)

At the end of the 1920s, it was unclear which of three schools in neuropsychology would gain hegemony within the Soviet Union. Thus far, methodology had been mainly mechanistic, but now the crucial question was what exactly should constitute the "materialist base" (Kozulin, 1984, p. 20) of Soviet science.

At the school that Bekhterev directed, research was being conducted on associative reflexes. After his death, in 1927, it was rumored that Bechterev had been neuropsychological consultant to the Kremlin rulers and that the official reports about the cause of death were clearly conflicting (Kozulin, 1984). Nonetheless, the theories offered by Bekhterev's school continued to be preeminent within most universities throughout the country. Pavlov, who had received the 1904 Nobel Prize for physiology for his work on the mechanisms of digestive secretion (Grigorian, 1974), was then in Leningrad. Determined to overthrow Bekhterev, Pavlov had begun in the 1920s to work on neuropsychology (*higher nervous activity*). In his theory of conditioned reflexes, he capitalized on his earlier, successful mechanistic approach. Until late in his life, Pavlov was critical of communism. Still, in an official decree (dated January 24, 1921), Lenin had praised Pavlov's work, emphasizing "the outstanding scientific services of Academician I. P. Pavlov, which are of enormous significance to the working class of the whole world" (The Nobel Foundation, 1997). Finally, Kornilov, whose Marxist "reactology" distinguished several "levels of reaction" (Van der Veer & Valsiner, 1991) was in Moscow. Kornilov's school was based explicitly on dialectical materialism and was therefore considered politically correct. However, studies of higher levels of reaction were always at risk of being denounced for "bourgeois tendencies".

At the end of 1930, the Bekhterev school lost its preeminence. One of its proponents was forced to admit that "we have witnessed certain attempts to base Soviet psychology on different psychological trends which are, in their essence and origin, avowedly bourgeois, instead of founding it on the philosophical heritage of Marx, Engels, and Lenin, Bolshevik experience, and the works of Stalin" (Kozulin, 1984, p. 59).

Clearly, the Soviet regime had taken control, and through 1950 no principled discussions on the concepts of scientific methodology were allowed: The concepts of science were to be derived directly from dialectical materialism (Kozulin, 1984). In Stalin's Soviet Union, despite the mass murders of the Kulaks, the fake trials, the terror of the purges, and the devastation of World War II, public disagreement was not permitted (cf. Aksyonov, 1995). However, whole generations of young intellectuals still genuinely wanted to build a communist science; they viewed the excesses of Stalinism as mistakes that eventually would be mended (Joravsky, 1989; Kozulin, 1984; L. Latash, March 1995, personal communication).

From 1930 through 1948, the fate of the schools of Pavlov and Kornilov remained undecided. It was Russian (not Soviet) nationalism, so typical of the period after World War II, that finally led to the canonization of Pavlov. In the early 1930s, there was no way that Bernstein could already have foreseen that development. Most of his articles that are available to us from the second period of his work contain, in one way or another, a rejection of Pavlov's (and Braune and Fischer's) simplistic mechanicism. The core of Bernstein's argument appears to be that Pavlov failed to understand the brain because he failed to understand its most important function, that is, the organization of movement. In 1935, when Bernstein published his now-famous article on coordination and localization, the 15th International Physiological Congress was held in Leningrad and Moscow in honor of Pavlov. At the time, Bernstein was working on a book, *Contemporary Studies in the Physiology of the Nervous System*, which contained a strong critique of Pavlov's theory. When Pavlov died in 1936, however, Bernstein refrained from publishing that manuscript (Feigenberg & Latash, 1996). In 1948, Bernstein received the Stalin prize for his 1947 book, *On the Construction of Movements*. In a manuscript that was written during the same period, "On Dexterity and its Development," he accused the adherents of conditioned reflexes of "carelessness" in their scientific reasoning (Bernstein, 1991/1996, p. 179), the strongest anti-Pavlovian statement that we have found in his translated work. Soon, however, attacks by neo-Pavlovians would silence Bernstein, and the manuscript was not published during his lifetime.

The only confirmation we have of Bernstein's awareness of problems surrounding political correctness is that the term *mechanics*, used so often in the first period of his work, had been replaced by *dynamics*. Whether that change arose from his genuine enthusiasm for the development of communist science, out of political expediency, or in a deliberate attempt to take over hegemony from Pavlov, remains to be established.

During this second period, Bernstein was strongly influenced by developments in Germany, where, in the life sciences, researchers were exploring alternatives for mechanicism and for vitalism, which is the idea that the nature of a living organism results from a vital force. In new approaches, the role of wholes was emphasized, there was a focus on hierarchical levels, and dynamical processes were being considered (cf. Von Bertalanffy, 1933/1962) Biological organisms were seen as functional systems, *function* and *system* becoming central concepts in the works of biologists and psychologists such as Anokhin (e.g., 1935), Köhler (e.g., 1924, 1933), Von Uexküll (cf. 1980), and Weiss (e.g., 1928). Bernstein incorporated many of their ideas into his own thinking (cf. Bongaardt, 1996).

In the international literature, some authors made connections between organismic biology and mathematical physics. Van der Pol and Van der Mark (1928), for instance, interpreted the heartbeat as a relaxation-oscillation, a phenomenon that synchronizes easily with external rhythms. Adrian and Buytendijk (1931) discovered in the medulla oblongata of the goldfish a neural oscillator that evidently was involved in the organization of breathing. Between the two World Wars, the mathematical physics of stable oscillations was developed mainly in the Soviet Union. Inspired by Poincaré, Lyapunov had started to study stability. That initiative was picked up by Krylov, who modeled the stability of ships. After an initial silence, the views of Lyapunov and Krylov became widely debated in the Soviet Union of the 1930s (cf. Grigorian, 1973a, 1973b, 1973c).

It was Andronov who laid the foundations of what is known today as the *theory of dynamic systems*. In 1937, Andronov and Chaikin published *Theory of Oscillations*. An important question in that book was how the state (x) of a system changes over time (dx/dt; the authors used the term $\dot{x}$). State changes may be a function of only the state of the system itself ($\dot{x} = f[x]$), or also of an external factor, modeled as time ($\dot{x} = f[x,t]$). Andronov and Chaikin defined a system as *autonomous* if it changes over time as a function of its own state only (Andronov & Chaikin, 1937/1949, p. 9). With their approach, it became possible to develop mathematical models for autonomous phenomena. By 1935, Bernstein had already addressed similar issues regarding the organization of movements.

Bernstein had moved, by 1935, from the Central Labor Institute to the Laboratory for Biomechanics of the Central Scientific Institute of Physical Culture, while also setting up laboratories for biomechanics in several other scientific institutions (Feigenberg & Latash, 1996). Shortly before the war, he became director of the movement laboratory of the Institute of Neurology, which had originated from the Institute of Experimental Medicine. In 1942, the latter Institute also gave rise to the Academy of Medical Sciences, where Bernstein was later elected as a corresponding member.

On June 22, 1941, Hitler attacked the Soviet Union. When German troops approached Moscow, Bernstein fled to Siberia, together with his wife, Beatrice, whom he had married in the 1930s, and his stepdaughter, Tatiana. Conditions were harsh, and they traveled to Tashkent upon an invitation from Bernstein's brother (Feigenberg & Latash, 1996). No experimental work could be done until after the war; Feigenberg's (1988) bibliography of Bernstein's works cites no publications for the years 1942–1944. Once back in Moscow, Bernstein shared in the atmosphere of general optimism (Feigenberg & Latash, 1996), while he met with recognition and success. At the same time, however, his enemies were preparing to attack. In 1949, Kreshtovnikov wrote in "Theory and Practice in Physical Culture" that "Bernstein violated the principles of the [Communist] party's approach and historical perspective . . . , displayed adoration of foreign scientists, . . . neglected the importance of the works by I. P. Pavlov" (cited in Feigenberg & Latash, 1996, p. 249). The days of success were over.

The Structure of Coordination

In the late 1920s, Bernstein had reported with some pride that his kymocyclographic analysis highlighted "the high degree of automation . . . mechanical simplicity and lawful structure" (Bernstein, 1927b, p. 789; see the previous discussion) of cyclical movements. In 1935, however, he came to the conclusion that "successive movements of cyclical nature *never exactly repeat themselves* [italics added]" (Bernstein, 1935/1967e, p. 48). He had made cyclograms of rhythmic movements on a stationary photographic plate and had seen that the metric details of the movement were different on each repetition. Because the details were never the same twice, straightforward inference from any observed movement via force to the central signal was not possible.

Bernstein then presented a mathematical analysis of how the state of a moving system changes over time. Torque, he assumed, changes as a function of joint angle, angular velocity, and muscle excitation. The "structure of muscle excitation" (Bernstein, 1935/1967e, p. 19) must at least in part be a function of the central signal. Mathematically, the implication of that assumption is that muscle excitation E must be modeled as a function of time, as in the following:

$$E = f(t). \tag{1}$$

If E were determined only by t, Bernstein reasoned, the actual movement would have no relationship whatsoever "to the local conditions operating in the system" (Bernstein, 1935/1967e, p. 18), as indeed is the case in "proprioceptive ataxia" (p. 19). Thus, in Bernstein's view, purely centralist models of control refer to pathological conditions. Muscle excitation must be partly a function of the state of the system itself:

$$E = f(x, dx/dt). \tag{2}$$

If E were determined only by joint angle and angular velocity, however, the system would suffer from "central paralysis" (Bernstein, 1935/1967e). Given that muscle excitation has a functional structure, Equations 1 and 2 must be combined.

> And so we are left with the hypothesis that the excitation of a muscle E must be both a function of time and a function of position and velocity, and must be described in . . . the form . . . $E(t, x, dx/dt)$ (Bernstein, 1935/1967e, pp. 18–19).

That is,

$$E = f(t, x, dx/dt). \tag{3}$$

That equation permits an "exceptionally simple translation into physiological terms" (Bernstein, 1935/1967e, p. 19). There is a *peripheral cycle of interaction* in which movement occurs whenever the "equilibrium in the force field is destroyed" (Bernstein, 1940/1967a, p. 62) by external forces or by the central signal. Within that cycle,

> changes in muscle tension bring about a movement and the movement affects the condition of the muscles by shortening or stretching them causing further changes in their tension. . . . [Consequently,] this form of interaction *does not presuppose a one-to-one correspondence between force and movement*, that is, . . . one and the same sequence of changes in forces may produce different movements on successive repetitions (Bernstein, 1940/1967a, p. 62).

At least two values independent of the equation itself (for example, position, velocity, the force field as a whole) are required for the solution of the differential equation that relates forces and movements. That requirement explains why, integrated with the peripheral cycle of interaction, a central cycle of interaction exists "in which the central nervous system constantly receives information as to the state of these independent parameters of integration and adapts its effector impulses in an exact relationship to the latter" (Bernstein, 1940/1967a, p. 63). The functional integration of those two cycles yields "*functional non-univocality* . . . between impulses and effects" (p. 105). That is, the same observable movement may be effected by different innervations, or the same innervation can lead to different movements. Bernstein inferred that the changing nonlinearities in the relationships between impulses and forces and movement preclude the mechanicistic idea of a central signal just "striking a piano key" (cf. Bernstein, 1935/1967e). He reminded the reader, as follows, of the failure of the steam organ:

> I would like to recall here the failure in 1923 of the invention of 'a symphony of whistles'. An attempt to convert steam whistles into a musical instrument with an organ keyboard failed because any given whistle could not be relied upon to sound the same on every occasion, and its pitch would vary with the pressure of steam, with the number of whistles sounded simultaneously, with the degree to which the steam-channel was clear, and so on, so that it was impossible to obtain a one-to-one correspondence between the keyboard, on the one hand, and the frequency of the tones obtained, on the other (Bernstein, 1935/1967e, p. 33).

However convincing that argument on the basis of changing nonlinearities was (and still is), Bernstein more often pointed to the degrees of freedom problem. In the old conception of control, the central signal was alleged to handle most if not all degrees of freedom. Bernstein's "revolt" (Kozulin, 1984, p. 62) was to argue that in motor control a continuous, circular flux of information is needed so that those degrees of freedom can be mastered.

On the basis of that reasoning, Bernstein, for the first time, presented the idea of a hierarchical organization of the movement systems. He distinguished, first, "in the supreme nervous organ an exact representation of what will later occur at the periphery" (Bernstein, 1935/1967e, p. 40); second, mediating processes of integration of the central signal and sensory information; and, finally, the biomechanical organization of the locomotor apparatus and the overt structure of the movements themselves. He referred to the latter two levels as *the coordination of movements* (Bernstein, 1935/1967e), which he later defined as "*overcoming excessive degrees of freedom of our movement organs, that is, turning the movement organs into controllable systems*" (Bernstein, 1991/1996, p. 41). Coordination is of a much more flexible nature than the traditional reflex. Degrees of freedom are taken together, now in this way and then in another, depending on the task and on the experience of the performer.

> When someone . . . first attempts to master the new co-ordination, he is rigidly, spastically fixed and holds the limb involved, or even his whole body, in such a way as to reduce the number of kinematic degrees of freedom which he is required to control. . . . Having mastered the first degrees of freedom the organism increasingly raises its ban on further degrees of freedom. . . . Here two successive stages of release may be observed. The first degree corresponds to the lifting of all restrictions, that is, to the incorporation of all possible degrees of freedom. . . . The second highest stage of co-ordinational freedom corresponds to a degree of co-ordination at which the organism is not only unafraid of reactive phenomena in a system with many degrees of freedom, but is able to structure its movements so as *to utilize entirely the reactive phenomena* which arise (Bernstein, 1940/1967a, pp. 108-109).

Movement coordination was seen both as a process (the neuromotor processes of integration) and as a structure (its observable form), just as organs can be understood as processes as well as structures (Bernstein 1940/1967a). The structure of coordination reveals itself in the "motor field" (Bernstein, 1935/1967e, p. 42), that is, the space in which movements take shape. In the motor field, there is no essential difference between producing an A or an A, just as "the recognition of the letter A does not require the presence of any metrical properties and is, on the contrary, entirely dependent on the presence of determinate topological cues" (Bernstein, 1935/1967e, p. 44). Like the perceptual field, the motor field is characterized by global topology rather than specific metrics:

> The co-ordinational net of the motor field must be regarded, in distinction to a net in Euclidean geometry, firstly as non-rectilinear, and secondly as oscillating like a cobweb in the wind. Its 'oscillation' does not, however, in every case proceed so far as to destroy topological relationships either of zero order (for example the category 'between') or of the first and perhaps even higher orders (Bernstein, 1935/1967e, pp. 48–49).

Because coordination "hints at the common action of separate elements" (Bernstein, 1935/1967e, p. 30), it cannot be based on "particular processes in individual neurons, but on the determinate *organization* of their common activity" (Bernstein, 1935/1967e, p. 30). Hence, in his notion of "localization" in the nervous system, Bernstein does not refer to individual neurons but rather to their common organization. Analogously, according to his view, the central signal is written in terms of the overall structure of the movement and not in terms of its spatial details, forces, or torques. Taking that notion as a starting point, Bernstein proposed to use a principle of equal simplicity, which holds that topologically similar movements are univocally related to a particular organization within the central nervous system and the brain.

Through the study of the movement coordination (both structure and process), Bernstein strove to gain insight into the "true categories" (Bernstein, 1935/1967e, p. 36) of movement organization that exist in the brain and, ultimately, to understand the organization of the brain itself.

Movement and the Brain

Bernstein did not reject Braune and Fisher's mechanicism and Pavlov's theory of conditioned reflexes all at once. The path leading to that conclusion was tortuous. Bernstein's articles are difficult to read, and they often contain remarks that lead away from the main argument or are even wrong. In his zeal to reject conditioned reflexes, for example, he emphasized so strongly the structural integrity of movement that he suggested that all the details of movement timing must be "organized within the required degree of accuracy a full second beforehand" (Bernstein, 1935/1967e, p. 24). Contrary to his own theory of coordination, Bernstein failed to argue here that the final temporal structure of the movement may be an emergent property of the movement system as a whole. In addition, after telling us that movements never exactly repeat themselves (Bernstein, 1935/1967e), he states, in an analysis of locomotion, that the "most rigid succession of all details is followed from cycle to cycle and these details are extremely repetitive for each subject" (Bernstein, 1940/1967a, p. 60).

Bernstein took inspiration from a great variety of sources, sometimes elaborating on the theories he borrowed, sometimes exploiting them in one or two of his articles, but he then remained silent about those theories in the rest of his work. In 1935, for instance, his notion of the "topology" of the overall structure of a movement was clearly derived from Gestalt theory; but from 1940 onward, he rejected Gestalt theory because of its lack of biological detail (Bongaardt, 1996). In 1940, he came under the spell of Weiss's (1928) hierarchical "organicism," but he then proceeded to change Weiss's anatomical hierarchy almost beyond recognition, ending up with the theory that the motor problem at hand compels the organism to choose, or even to construct, a particular level of coordination for its solution (Bernstein, 1991/1996).

Amid all those details it is sometimes difficult to keep track of the overall topology of the theory that Bernstein was in the process of developing. Sometimes one sees him as the originator of a new theory of coordination (Greene, 1972; Turvey, 1977). At other times, he seems to be an important neuroscientist working on the organization of the brain (Feigenberg & Latash, 1996; Pickenhain, 1988). He did both at the same time, however, and he regarded his work as an integrated enterprise, as revealed in the title of his article, "The Problem of the Interrelation of Coordination and Localization" (Bernstein, 1935/1967e). In fact, in all of his publications from the second period of his work, he had something to say about movement and about the brain; but through a variety of research strategies, he in fact questioned their interrelationship.

With great enthusiasm, Bernstein studied a wide variety of movements in sports, daily life, labor, and in human pathology. After presenting an elaborate analysis of the kinetic details of human walking, he concluded that the reflex arc cannot exist, and that the organization of movement requires reflex rings (Bernstein, 1940/1967a). He used the newly developed technique of electroencephalography, and he claimed that the electrical oscillations of individual neurons necessarily organize into groups with synchronized frequency and phase. He concluded that brain slow waves are derived from old organizations of movement (*paleokinetics*) and phylogenetically precede the emergence of the striated muscles, whereas brain fast waves (*neokinetics*) evolved on top of the older level, like a telegraph wire system, although they are still controlled by the older level. That notion implies, Bernstein contended, that "preliminary adjustments and adaptations proceed on the background of intense ongoing activity rather than on the background of general rest of the organism" (Bernstein, 1945/1998, p. 297).

In 1947, Bernstein received the State prize for his book, *On the Construction of Movements* (that as yet untranslated work is summarized in Bernstein, 1991/1996). Every skill, he claimed, arises in answer to a particular motor problem; the task of the organism is to assign control of sensory corrections to a particular level of coordination. First, at the paleokinetic level, muscle tone, that is, the preparedness of the organism, is maintained. Synergies, are dealt with at the second level, the level of muscular-articular linkages. That level is "well-suited for the *accumulation of life experience*, for building new coordinative patterns and storing them in the treasury of motor memory" (Bernstein, 1991/1996, p. 128). The level of space "emerged when vertebrates moved onto solid ground and into the air and also acquired extremities" (Bernstein 1991/1996, p. 132). At the space level, movements "possess a somewhat business-like dryness and do not involve large groups of muscles. They are, so to say, the chamber music of the muscle" (p. 135). The highest controlling level that Bernstein analyzed is that of action, where there are "whole sequences of movements that together solve the motor problem. . . . All the movements . . . are related to each other by the *meaning* of the problem" (p. 146).

In his 1947 theory, there were also clear signs of the emergence of new issues that would occupy him for the rest of his life. In his manuscript, "On Dexterity and Its Development," he defined *dexterity* as the ability to solve motor problem in always novel ways. He stressed that such "*motor wits* in unexpected situations" (Bernstein, 1991/1996, p. 177) are principally different from coordination. Dexterity, Bernstein argued, is a typical characteristic of animals with a well-developed cortex. Such "highly developed animals demonstrate higher variability over the centuries" (Bernstein, 1991/1996, p. 47). That variability is reflected not only in their motor behavior but also in the brain, in which "telegraph wires" send "blind spikes" on top of the "bioelectrical glitter" of the underlying paleokinetics (cf. Bernstein, 1945/1998).

After the Neo-Pavlovian Craze (1954–1966)

In 1924, Vygotsky presented his views to the Second Russian Psychoneurological Congress in Moscow. Vygotsky intrigued the audience by challenging the idea that behavior consists of reflexes. In his activity theory, he held that human behavior is "structured and organized according to specifically human social goals" (Kozulin, 1984, p. 105), and that those can be scientifically studied.

> When Vygotsky appeared on the scene, world psychology was in a state of crisis. Psychology was divided into two isolated fields. On the one hand, there was the research of Ivan Petrovich Pavlov and Vladimir Mikhailovich Bechterev on the physiological mechanisms that underlie behavior. How-

> ever, this work did not provide an adequate method for the analysis of the more complex forms of man's conscious mental activity, such as abstract thinking, deliberate remembering and voluntary attention. . . . Young Soviet psychologists wanted to take the complex forms of man's conscious activity as their subject matter, but they wanted to study the area objectively; to produce a scientific explanation of the development and laws of the higher forms of mental activity. . . . It was Vygotsky who . . . felt that attempts to reduce mental activity to a system of reflexes were the wrong way to proceed (Luria, cited in Cole & Cole, 1971, p. 82).

Bernstein learned about the psychology of activity as early as 1924 through his work at the Moscow Institute of Experimental Psychology, where he collaborated and co-published with, among others, Leontiev, Luria, and Vygotsky (cf. Feigenberg, 1988).

In the early 1930s, research in the field of psychology of activity ran into problems, allegedly because it was focused too much on the human individual and on social behavior as a higher, "cultural function" separated from lower, "natural functions" (Kozulin, 1984; cf. Van der Veer, 1990; Van der Veer & Valsiner, 1991). Vygotsky's publications were blacklisted, but as long as they did not mention his name, his students and colleagues were able to continue their work relatively undisturbed in faraway Kharkov. There, they studied the importance of action for the development of mental functions. One of Vygotsky's colleagues, Leontiev, recognized the relevance of Bernstein's "multilevel theory of movement coordination, suggesting that it could provide a . . . basis for his own theory of activity" (Kozulin, 1984, p. 70).

Bernstein's rejection of the stereotyped reflex as a neurophysiological basis for movement behavior was conceptually close to the views expressed by researchers of the Kharkov school. On the other hand, much of his recognition came from practical applications of his ideas on the construction of movements—in sports, in the production of artificial limbs, and, later, in the training of cosmonauts (Kozulin, 1984). However, the year he received his State prize not only brought him acclaim, it also shattered his relative safety.

The main issue discussed at the 1948 biology session of the Academy of Science was whether behavior results from education or from genetically fixed properties. It was claimed that one could create new properties at will by manipulating external conditions (Lysenko, 1934–1952/1954; Regelmann, 1980). To insiders, then, it came as no surprise that neo-Pavlovian conditioning gained supremacy in neuropsychology as a result of the 1950 Joint Session with the Academy of Medical Sciences.

During the 1949 anti-Semitic campaign, Bernstein was accused of relying on foreign authors; and in 1950, he was officially denounced in *Pravda*, the party-line newspaper (Feigenberg & Latash, 1996). Allegedly, Bernstein's conclusion that motor performance does not repeat itself was considered incompatible with the neo-Pavlovian theory of the conditioned reflex. He was fired, and the doorplate of the Bernstein Laboratory for Biomechanics was hammered to pieces (Feigenberg & Latash, 1996). Living on his stipend from the Academy of Medical Sciences, Bernstein spent most of his time at home, on Shchukin Street in Moscow. There, Bernstein and his wife took daily doses of morphine at about 2 o'clock every afternoon (the morphine was acquired through the help of friends, but eventually by prescription, with official permission from the Ministry of Health). They did so until the end of their lives (L. Latash and V. Zatsiorsky, March 1995, personal communication).

In September 1953, Khrushchev came into power, and in 1954, Bernstein was allowed to work in the Institute of Neurology—now, as a senior scientist, entitled to one assistant. He would walk there in the early morning, returning home around noon. In 1956, Bernstein retired. By then, most of his friends had lost their confidence in the Soviet system, but Bernstein continued to refer to Marxism-Leninism in his public speeches. Actually, the specter of neo-Pavlovianism would never leave him, and his friends often met behind closed curtains when discussing his works (L. Pickenhain, personal communication). Being publicly, anti-Bernstein was a way to survive; and, for a long time, paying lip-service to neo-Pavlovianism was considered expedient (V. Zatsiorsky, March 1995, personal communication).

Never having admitted his so-called mistakes (Kozulin, 1984), Bernstein created a *physiology of activity* without explicitly referring to Vygotsky, at least not in his translated articles. In 1962, Bernstein presented his non-idealistic alternative to neo-Pavlovian reflexology at the All-Union Meeting on Philosophical Problems of the Physiology of Higher Nervous Activity and Psychology (Kozulin, 1984). During the same year, Gastev and the Central Labor Institute were officially rehabilitated through the initiatives of Berg, then the leading Soviet cyberneticist (Bailes, 1977).

At that time, in Soviet politics, workers in mechanistic cybernetics were urged to provide and develop the knowledge needed to control the economy and enhance its success. By 1957, Bernstein had been invited to a seminar on cybernetics by Lyapunov, the son of the famous mathematician (cf. Lyapunov, 1960). Bernstein was also one of the influential contributors to a 1962 conference on Biological Aspects of Cybernetics, where he argued that an integrated cybernetics of control and optimization was to be realized by the "science of sciences, mathematics" (Bernstein, 1962/1963, p. 69). At that point, his agenda coincided with that of Berg, the organizer of the conference. In his own contribution, Berg (1962/1963) literally copied phrases from Bernstein's 1957 work. Fundamentally, however, the two must have disagreed, because Berg defended reactive mechanics, and Bernstein promoted the notion of active dynamics. Scientifically, Bernstein stood closer to the influential, independent mathematician Gel'fand and to the theoretical physicist Tsetlin, who also contributed to the conference.

Fascinated by Bernstein's work, Gel'fand and Tsetlin created a movement laboratory, first in the Biophysics Institute, later in the Institute for Problems of Information Transmission. Movement science grew unprecedentedly through the work of Bernstein's students (cf., e.g., Gel'fand, Gurfinkel, Fomin, & Tsetlin, 1971). An important role in that development was played by Gel'fand, who initiated a seminar on a wide range of topics, inviting only the best in their respective fields. Some of the meetings were held at Bernstein's home, where he preferred to have discussions with only a select few. Bernstein continued to meet his friends at home until he died in 1966 from renal cancer.

Toward a Physiology of Activity

After the neo-Pavlovian craze period, Bernstein attempted to proceed beyond coordination.

> The period of struggle towards the recognition of the biological importance, the reality and the generality of the principle of cyclical regulation of life processes is now behind us. . . . The present author gave an account of the application of these concepts to the problem of the co-ordination of movements in a report in 1929 [not translated] in which this principle was given a general mechanical foundation . . . , and in 1935 [Bernstein, 1935/1967e] he reduced it in general terms to differential equations. The debate, in these initial stages, was conducted sharply, but now seems to be over. . . . Clearly, now is the time to look forward (Bernstein, 1957/1967f, p. 114).

In the psychology of activity, advanced by researchers of the Kharkov school, the decisive role that experience plays in the development of higher functions such as language was emphasized. Without so much as referring to the Kharkov school, Bernstein, in his 1957 article, aimed at developing a physiology of motor acts that emphasized the reciprocal relationship between the moving organism and its environment. Motor problems arise "out of the external environment" (Bernstein, 1957/1967f, p. 115), upon which the organism "actively operates" (p. 114), and from which it receives "sensory feedback" (p. 116). In essence, biological activity implies "the cognition of the surrounding world through action and the regulation of action within it" (p. 124).

Contrary to what "many thinkers in the Western world" (Bernstein, 1957/1967f, p. 124) had said, Bernstein held that the active interaction of the organism with its environment takes an objectively knowable world as its starting point.

> Here the degree of *objective reality* of the information is a decisive prerequisite for the success or failure of the action to be performed. During the entire course of phylogenesis of living organisms natural selection inexorably sifted out those individuals in which the receptors controlling motor activity operated like a curved mirror. . . . Each meaningful motor directive demands not an arbitrarily coded, but an objective, quantitatively and qualitatively reliable representation of the surrounding environment in the brain. . . . This also leads to knowledge through action and *revision through practice* which is the cornerstone of the entire dialectical-materialistic theory of knowledge, and . . . serves as a sort of biological context for Lenin's theory of reflection (Bernstein, 1957/1967f, pp. 119–120).

Objective information allows for the planning of behavior within a changing environment. For that reason, Bernstein argued, there must exist "a control element, which conveys to the system in one way or another the *required value* of the parameter which is to be regulated" (Bernstein, 1957/1967f, p. 129). The required value is then compared with the actual value, and the "interacting background levels of co-ordinational control" (Bernstein, 1957/1967f, p. 136) are set to work to overcome the differences.

Bernstein's conception of a control element, or "leading level" (Bernstein, 1967/1988b, p. 130), led him to work on control, optimization, and planning. He came to regard his earlier work, for which he now coined the term "the physiology of coordination" (cf. Bernstein, 1957/1967f), as a deterministic and technical aspect of movement. In the 1960s, he wished to develop a comprehensive physiology of activity (cf. Bernstein, 1961/1967h).

During the 1960s, Bernstein's theory of the physiology of activity gradually unfolded; he aimed at nothing less than a non-metaphysical, naturalistic understanding of life. Animals "pursue goals" (Bernstein, 1965/1988a, p. 238), and those goals must have natural origins. Darwinian variation and selection is the natural mechanism that forms the basis of activity, that is, the "*anti-entropic structuring of a self*" (p. 238). In that sense, activity is not restricted to movement behavior but can be found at all levels of life, from molecular complexes to brains "in all possible manifestations of vital activity both in onto- and morphogenesis, and in all forms of interaction of the living organism with its surroundings" (Bernstein, 1969, p. 120).

Bernstein considered movement to be the most typical manifestation of activity. Hence, an advanced understanding of movement could serve as the basis of a general theory of activity in living organisms.

The fact that movements are goal directed appears to imply that they are controlled by what is to happen. However, the future cannot determine the present. The control element must therefore rely on a *model* (Bernstein, 1961/1967h, p. 157) of the future. The hallmark of modeling is selecting relevant information—in the case of models of the future, "the essence of the matter . . . as yet unrealized" (Bernstein, 1961/1967h, p. 150). Moving organisms cannot afford to wait until the future materializes. They must take the initiative, thereby incorporating a measure of uncertainty into their motor acts. The structure of the past-present maps onto the brain and the resulting model is probabilistically extrapolated so as to predict the "course of events in the environment" (Bernstein, 1962/1963, p. 68; cf. Bernstein, 1960/1975). Hence, an important issue for physiologists and mathematicians alike is to determine which forms of extrapolation are actually used by the nervous system.

> Considering the lower, purely biomechanical, types of regulation which antecede a particular action by a minimal period of time, we apparently encounter extrapolation of the same type as that incorporated in a Taylor series with the use of two primary derivatives of information, that is, data from the joint and muscle signaling systems. (This is sometimes described as gradient extrapolation.) Considering more complex and meaningful types of plans for movements, such as may require reprogramming during their course, the higher

> co-ordinational brain systems and the synthetic processes involved will be found to include forms of probabilistic extrapolation among their equipment, and these will doubtless include such methods of active sampling as have been formulated and described in the contemporary mathematics of estimation as methods of non-local search (Bernstein, 1961/1967h, p. 161).

Bernstein's conception of models of the future was aligned with the mathematics of Gel'fand and Tsetlin. To capture search strategies in biological systems, Gel'fand and Tsetlin (1962; cf. Gel'fand, Gurfinkel, & Tsetlin, 1962/1963) distinguished between "essential" and "nonessential" variables. Bernstein contended that finding the essential variables of a movement-to-be implies exploring the topology "of the *future requirements of the individual*. . . . of that which is not yet, *but which must be, the case*" (Bernstein, 1967c, p. 187; cf. Bernstein, 1962/1963, 1962/1967i, 1965/1988a, 1969). When the solution-in-principle to a motor problem is found, a resulting plan of action serves to realize the movement "whatever might happen, overcoming obstacles and, if necessary, reprogramming during operation" (Bernstein, 1962/1967i, p. 179). Moreover, the nonessential variables are not merely "yieldingly adaptive" (Bernstein, 1969, p. 122) during movements, but

> those aspects of the remaining variability that have no reactive adaptive value can justifiably be looked upon as search-variability, in which the active exploration of the environment, its gradients, the optimal way to act, etc., come to the fore (Bernstein, 1965/1988a, p. 245).

During the last years of his life, Bernstein's central concern was the match between mathematical models and a naturalistic understanding of goal-directed behavior. "Biologists understand the problem but lack the mathematical skills, and mathematicians have the skills but don't understand the problem" (Bernstein, 1965/1988, p. 246; cf. Lyapunov, 1972). He came to realize that cybernetics was insufficiently equipped to deal with all relevant aspects of life and that, possibly, the "honeymoon of this union between automatic processes and physiology is over" (Bernstein, 1967c, pp. 185–186). Cybernetics may capture "self-programming automata" (Bernstein, 1967c, p. 187) that are able to estimate what will happen but cannot model what has to happen.

Self-programming automata are perhaps good enough for coordination, but in the third period of his work Bernstein conceived of a biology of activity in which the goal-directedness of movement was modeled as a natural phenomenon. Eventually, Bernstein believed, a comprehensive mathematics would be formulated that allows for understanding material systems that stochastically explore their goals, select one to pursue, and then find ways to realize this goal.

> As soon as we are equipped with the real, adequate mathematical apparatus (maybe already in the near future) we can assume that biology and biocybernetics will fuse into one synthetic science, and will thereby reach a new, higher level (Bernstein, 1965/1988, p. 241 [authors' translation]).

Bernstein was almost 70 years old when he expressed that mathematical dream. In that final year, he invited his students and colleagues to his home. His message was that work should continue, no matter what happened (Feigenberg, 1978). Shortly before he was to present an article titled, "The Immediate Task of Neurophysiology in the Light of the Modern Theory of Biological Activity," at the 1966 XVIIIth International Congress of Psychology in Moscow (cf. 1969), Bernstein died.

Discussion

It would be relatively easy to conclude that Bernstein, throughout his life, was inconsistent, jumping onto any "bandwagon" that passed by. First, he was inspired by Braune and Fischer, and then he rejected their work. He was taken with Gestalt theory in 1935, only to abandon that theory a few years later. Although many of his ideas were derived from Weiss, in his later work Bernstein changed Weiss's theory beyond recognition. Even Pavlov must have been a source of inspiration to Bernstein, but he spent much energy attacking Pavlov's theories. In the last phase of his life, he plunged into the latest fashion in psychology research, that is, into typical Soviet theories of activity.

If we accept such a conclusion, however, we will completely fail to do justice to the tenacity with which Bernstein pursued his quest. In fact, Luria (1987) emphasized that Bernstein "was a rare case of a scientist who practically devoted his whole life to one problem: The physiological mechanisms of human movements and motor actions" (p. 85). Although, in its silence about Bernstein's fascination with the brain, Luria's statement is one-sided, we wholeheartedly agree with his tone.

Nevertheless, the overall structure of Bernstein work renders it understandable that different authors have been inspired by different aspects of his theory. The thrust of his work is in its unity, which emerges in our reconstruction of its development.

The Stalinist era produced one of the greatest tragedies in the history of science, and Stalinism was a horrifying instance of what human beings can do to other human beings—actions that we certainly cannot condone. During Stalin's Soviet Union, people were forbidden to think in terms of a soul or to work in dualistic schemes. Nonetheless, a whole generation of young intellectuals was forced to face the challenge to come up with natural models for those phenomena that traditionally had been explained by invoking the supernatural (cf. Joravsky, 1989). It is a matter of established historical fact that this generation took up the challenge. So did Bernstein.

During the first phase of his work, Bernstein understood that detailed measurement of movement was a necessary prerequisite to advancing the field. He spent about 10 years on the development of his kymocyclography, without bothering much about theory; his analysis with Popova of piano playing is the only exception we know of. He then undertook to unravel the consequences of his thesis that the organization of the brain is reflected in the structure of coordination. In so doing, he developed a theory of the brain that was much more sophisticated, and realistic, than any theory previously offered in the Soviet Union (cf. Sporns & Edelman, 1998). At the same time, he revolutionized the science of movement by showing, beyond doubt, that old theories (the piano key metaphor) were necessarily wrong, and that the organization of movement should be understood as the reciprocal attunement of several simultaneous kinetic and informational processes. It took him more than a decade to bring that idea to full fruition, after which it became clear that his theory could accommodate reactive phenomena but failed to say anything about the fact that animals are active, that is, that they take initiatives. Bernstein acknowledged that higher animals are more active than lower ones, and that higher animals possess a well-developed cerebral cortex. Hence, he argued that the formulation of goals and the search for ways to solve motor problems are functional properties of the cortex. Still, Bernstein realized that those higher phenomena were of such an abstract nature that no one could expect an explanation to come from neurophysiology alone. He therefore turned to mathematical modeling of planning and to searching for solutions.

Bernstein may have entered the field of movement science in order to understand the brain. It is clear that he used his understanding of the brain to advance the understanding of movement. His enthusiasm for all possible manifestations of biological movement led to his receiving recognition in the Soviet Union. Thus, in answer to our first question concerning the thrust of Bernstein's theory of movement behavior, we conclude that Bernstein's outstanding contribution to the sciences of the 20th century consisted of his conception of biological movement as a natural phenomenon, encompassing the interactions between the brain, the movement system, and the natural and cultural environments.

Bernstein's Heuristics

Bernstein wanted to include all the relevant aspects of biological movement into one coherent theory of movement behavior. We contend that Bernstein realized, around 1935, that it would be impossible to avoid the notorious problem of relating levels that each require a different language of description. In the quantum mechanics of his time, for example, particles and waves were considered complementary, that is, both levels of description are needed, although the two levels are not reducible one to another (cf. Pattee, 1993; Penrose, 1989). In his 1935 article, Bernstein stated that quantum mechanics is difficult to understand but that there is no reason to believe that true theories have to be easy.

Despite such problems, Bernstein displayed optimism, perhaps even overconfidence. In 1965, he presented his dream that all aspects of motor coordination and planning would finally merge into one comprehensive science of a mathematical nature. Given the history of mathematics in the 20th century, he could have known that such completeness cannot exist in formal theories (cf. Rosen, 1991). However, Bernstein exploited problems; he did not avoid them.

We suggest that Bernstein exploited the problem of relating all relevant levels in the organization of movement by repeatedly shifting focus: His work was mainly mechanistic in the 1920s; dynamic in the 1930s and 1940s; cybernetic and thus mechanistic again in the 1950s; and finally naturalistic with regard to planning and thus dynamic again in the 1960s. That shifting pattern reveals something more systematic than merely the pursuit of any attractive topic as it presented itself.

Perhaps the *nomenklatura* forced Bernstein into that pattern. At first sight, such a picture appears to fit: In the 1920s, Gastev forced mechanicism upon his Institute. After 1930, mechanicism was banned from science. Then again, after the neo-Pavlovian craze, Berg's cybernetics became important. In the last period of Bernstein's work, he easily affiliated with the influential work of Gel'fand. Nevertheless, the idea that he followed the pathway of mere political expediency is not tenable. Bernstein's 1929 analysis with Popova contradicted the then-dominant mechanicism. His rejection of purely central control in 1935 must have involved political risk. Berg may have been influential in rehabilitating Bernstein, but Bernstein's concept of the physiology of activity was contrary to Berg's mechanistic ideas of control. In addition, in Bernstein's final theory, he challenged scientists to develop formal models of how animals set their own goals.

Whether his tendency to be a dissident can be understood in terms of his personality is not important to the present argument. It is the heuristic value of Bernstein's repeatedly shifting focus that is important. Some of the major shifts of focus appeared even within a single article, particularly in the second period of his work; during that period, he attempted to focus on two different phenomena in each of his articles, that is, the structure of coordination and the organization of the brain. One could say that Bernstein progressed like a skater: He moved forward by pushing to one side first and then to the other.

Our second question concerns the relevance of Bernstein's work to the contemporary sciences of movement. Our answer is threefold: First, it remains important to model movement as a natural phenomenon and to refrain from dualistic schemes or from "taking loans" on intelligence (Dennet, 1987). Second, the outline of the theory he created is still valid and challenging. To make the movement system controllable, the organism needs coordination. Control itself can only be understood in terms of a motor problem (a task, for example, or a goal). Given such a problem, the cortex stochastically searches for an essential variable. The essential variable is then used in control, whereas coordination is relegated to the lower levels. Third, we contend that Bernstein's legacy to movement science is a methodological one. His way of asking questions and reaching for the answers, that is, his shifting-focus heuristic, ensured that he could continue his quest for understanding movement while avoiding the pitfalls of the past. Bernstein was never theoretically dogmatic in trying to understand a particular aspect of human movement. By formulating goals just out of reach, and by pushing off from established progress, Bernstein maintained a creative tension within the science of movement. He simply knew how to move ahead.

Epilogue

Neither the Academy of Sciences nor the Academy of Medical Sciences wanted to host Bernstein's funeral (L. Latash, 1995, personal communication). Permission to conduct the funeral was obtained by the Institute of Higher Nervous Activity, whose director, Asratyan, had been one of the leaders of Soviet neo-Pavlovianism. Thus, as fate would have it, Pavlov's portrait was staring down at Bernstein's coffin. The elite of neurophysiology and Gel'fand's group filled the small hall to capacity. Gel'fand spoke, emphasizing Bernstein's genius and modesty, and read the following poem by Boris Pasternak (1964, p. 70):

It is not seemly to be famous:
Celebrity does not exalt;
There is no need to hoard your writings
And to preserve them in a vault.

To give your all—this is creation,
And not to deafen and eclipse.
How shameful, when you have no meaning,
To be on everybody's lips!

Try not to live as a pretender,
But so to manage your affairs
That you are loved by wide expanses
And hear the call of future years.

Leave blanks in life, not in your papers,
And do not ever hesitate
To pencil out whole chunks, whole chapters
of your existence, of your fate.

Into obscurity retiring,
Try your development to hide,
As autumn mist on early mornings
Conceals the dreaming countryside.

Another, step by step, will follow
The living imprint of your feet;
But you yourself must not distinguish
Your victory from your defeat.

And never for a single moment
Betray your credo or pretend
But be alive—this only matters—
Alive and burning to the end.

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